Instrumental Variable Estimation for Duration Data: A reappraisal of the Illinois Reemployment Bonus Experiment

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Abstract

In this article we focus on time-to-event studies with a randomised treatment assignment that may be compromised by selective compliance. Contrary to most of the extensive literature on evaluation studies we do not consider the effect of the treatment on some average outcome but on the hazard rate. In time-to-event studies the treatment may vary over time. Another complication of duration data is that they are usually heavy censored. Censoring limits the observation period, but is not a feature of the treatment program. Therefore, a natural choice is to relate the treatment to the hazard rate. We show that even if the compliance is selective, we can still use the randomisation to estimate the impact of the program corrected for selective compliance on the hazard. The only requirement is that participation in the program is affected by a variable that is not correlated with the baseline duration.

We develop an Instrumental Variable estimation procedure for the Generalized Accelerated Failure Time (GAFT) model. The GAFT model is a duration data model that encompasses two competing approaches to such data; the (Mixed) Proportional Hazard (MPH) model and the Accelerated Failure Time (AFT) model. We discuss the large sample properties of this Instrumental Linear Rank Estimation and show how we can improve its efficiency. The estimator is used to re-analyze the data from the Illinois unemployment bonus experiment.

Key Words: Semi–parametric, duration model, endogenous treatment, instrumental variable.

JEL codes: C41, C14, C24, J64

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1 Introduction

In recent years, social experiments have gained popularity as a method for evaluating social and labor market programs (see e.g. Meyer (1995), Heckman, LaLonde, and Smith (1999) and Angrist and Krueger (1999)). In experiments the assignment of units to the intervention can be manipulated. If assignment is random, the average impact of the intervention can be estimated. However, a randomized assignment may be compromised, if the units can refuse to participate, either by dropping out, if they are to receive the intervention, or by obtaining the intervention, if they are in the control group. If this non–compliance to the assigned treatment is correlated with the outcomes in the treatment or control regimes, the observed effect of the intervention is a biased estimate of the intervention effect.

Even if the compliance is selective, we can still use the randomization in the assignment to the intervention and control groups to estimate the impact of the program correcting for selective compliance. This is the subject of this article. The methods that we propose can be used in all programs with selective participation. The only requirement is that participation in the program is affected by a variable that is not correlated with the potential outcomes. We focus on an outcome variable that is the waiting time to some event, e.g. unemployment duration.

Most of the evaluation literature has focused on static interventions, *i.e.* interventions that are administered at a particular point in time or in a particular time interval. An example is a training program. For instance, Ham and LaLonde (1996) study the effect of the experimental JTPA program, a training program directed at the unemployed, on unemployment spells. In that case the waiting time is an outcome variable like any other. If the outcome is a waiting time, or in general a time series, the intervention can be dynamic, *i.e.* it can be switched on and off over time. Examples are the unemployment insurance experiments (see Meyer (1995) for a survey) in which the unemployed receive a cash bonus if they find a job in a specified period. Another example is a temporary cut in unemployment benefits of unemployed individuals who do not expend sufficient effort to find a job (see Abbring, van den Berg, and van Ours (1997) and Van den Berg, van Ours, and van der Klaauw (1998) for The Netherlands and Ashenfelter, Ashmore, and Deschêne (1999) for the U.S.). As a final economic example we mention the studies on the effect of monitoring to the unemployed workers by employment offices (see Gorter and Kalb (1996) and Van den Berg and van der Klaauw (2000) for The Netherlands, Dolton and O'Neill (1996) for the so–called Restart experiments in the UK). The monitoring programs consist of one or more interviews of the (long termed) unemployed to counsel and advice them on effective job search.

Economic models for durations, *e.g.* search models, often have direct implications for the hazard rate. In general, the intervention may even depend on information that accumulates during the evolution of the event. With a time-varying intervention, the effect of the intervention becomes dependent on the outcome. Relating the time-varying intervention to the hazard seems the natural solution. Another reason to consider the effect of an intervention on the hazard rate is that duration data are usually censored. Censoring limits the observation period, but is not a feature of the program. Hence, the estimated effect should be independent of the censoring time. Because the hazard rate is invariant to censoring, it is natural to relate the intervention to this quantity.

Two competing approaches to the estimation of the effect of a time–varying treatment on survival has been the (Mixed) Proportional Hazard (MPH) model (for a recent survey see Van den Berg (2000)) and the Accelerated Failure Time (AFT) model (see a.o. Kalbfleisch and Prentice (1980), Brännäs (1992), Keiding, Andersen, and Klein (1997) and Klein and Moeschberger (1997)). In the Mixed Proportional Hazards (MPH) model the hazard is written as the product of the baseline hazard, a non-negative regression function, and a non-negative random variable that represents the covariates that are omitted from the regression function. In an accompanying article, Bijwaard and Ridder (2000), we introduced the Generalized Accelerated Failure Time (GAFT) model (see also Ridder (1990) and Bijwaard (2001)), a generalization of the AFT models that also includes the MPH model.

Using the actual treatment indicator as a normal covariate in the model will give biased results of the treatment effect if some of the individuals do not comply with their assigned treatment. The problem is that even if the intervention has no effect on the hazard the treatment parameter may not have a causal interpretation, because those who comply with their assigned treatment differ in observed and unobserved characteristics from those who do not comply. One could ignore the post–randomization compliance and rely on the analysis of the treatment assignment groups. This intention–to–treat estimator suffers from an error –in–variable bias.

We propose an Instrumental Variable (IV) method for GAFT models that adjusts for the possible endogeneity of the intervention, without suffering the problems of the Intention–to–treat method. We develop an estimation procedure that collapses to the Linear Rank Estimator procedure for Generalized Accelerated Failure Time (GAFT) if there is no instrumenting. This is accomplished by using a model that transforms the duration such that for the population parameters, this transformed duration is independent of the instrument.

The GAFT models with instrumenting are a generalization of the Rank–Preserving Structural Failure Time Models (RPSFTM) models of Robins and Tsiatis (1991) (see also Mark and Robins (1993b)). Both models consider a transformation of the duration time to identify the treatment parameters, but the GAFT models allow for an extension of the transformation. The main drawback of the RPSFT–models is that they assume a latent baseline duration time exists, representing the unit's survival time had the intervention always been withheld. This implies that if two individuals have identical durations and observed treatment and covariate histories then they would have had identical durations had they never been treated. The GAFT models are not rank preserving and therefore do not imply this strong non–interaction.

The Two Stage Linear Rank estimator (2SLR), we proposed in Bijwaard and Ridder (2002), is related to the Instrumental Variable Linear Rank Estimator (IVLR) we suggest in this article. The 2SLR restricts the transformation to an MPH representation and requires preliminary estimates of the baseline hazard. The 2SLR is only applicable if there is full compliance in the control group, because only then the preliminary estimates are identified. The analysis in this article does not impose the MPH assumption, nor does it require that the control group is totally excluded from treatment.

The existence of endogenous covariates implies (possible) dependence between the transformed duration and the censoring time. This implies that the IVLR estimator, which exploits the independence between the transformed durations and the instruments, may give biased results. If the censoring is part of the study design or a consequence of administrative rules, we can often make the assumption that the (potential) censoring time is known at the start of the study. Then, we can modify the GAFT transformation such that this modified transformation and the instruments are independent. Then, the IVLR estimator on this modified transformation leads to consistent estimators.

The estimator is used to re–analyse data from the Illinois unemployment bonus experiment. These data have been analysed before with increasing sophistication by Woodbury and Spiegelman (1987), Meyer (1996) and in Bijwaard (2001). In this experiment a group of individuals who became unemployed during four months in 1984 were divided at random in three groups of about equal size: two treatment groups and a control group. The unemployed in the claimant bonus group qualified for a cash bonus if they found a job within 11 weeks and would hold this job for at least four months. In the other treatment group, the employer bonus group, the bonus was paid to their employer. The members of the two treatment groups were asked whether they were prepared to participate in the experiment. About 15% of the claimant bonus and 35% of the employer bonus groups refused participation. Here we focus on the importance of extending the RPSFT–models to the GAFT models for the re–employment data. Indeed we find that evidence that the RPSFT–model is not the correct model for this application.

The outline of the article is as follows. Section 2 provides a discussion on the GAFT model with endogenous covariates. If the interventions are randomly assigned, this model reduces to the GAFT model. We explain the interpretation of the parameters of the model and discuss the problems of endogenous censoring. Within the counting process framework, the asymptotic properties of our estimator, which is introduced in section 3, can be derived using martingale theory. We discuss the consistency and asymptotic normality of the estimator. Section 4 discusses an empirical application of the IVLR estimator of the GAFT model with instruments.

2 The Generalized Accelerated Failure Time Model with endogenous Covariates

It can rarely be defended that a study on unemployment durations includes all the relevant characteristics of the individuals looking for a job. Consider, for example, the study on the effect of UI sanctions. Sanctions such as a temporary cut in unemployment benefits may be applied if a UI benefit recipient does not act according to certain rules concerning search behaviour and registration. The intention of a sanction is to change the behaviour of the benefit recipient, so that in the future she will comply with the rules. Because these rules are supposed to stimulate the benefit recipients to search actively for employment and because a cut in benefits itself gives an incentive to search more intensively the sanctions are expected to increase the re–employment hazard.

Suppose it is possible to observe the number of job applications in a particular time period of an unemployed individual. The number of applications has certainly an effect on the re–employment hazard. Since a frequently used reason for giving a sanction is an insufficient number of applications, it also influences the probability to get a sanction. However, the non (or partially) observed motivation to return to work has an impact on both the sanction probability and the number of job applications. The motivation to return to work is then an unmeasured confounder and the estimated treatment effects give biased results of the effect of the sanction. Note that both conditioning on the number of applications and ignoring it completely will induce a bias in the estimated effect of a sanction.

A way of adjusting for a correlation between the error and a covariate is the conventional instrumental variable assumptions of instrument–error independence and an exclusion restriction. A familiar example of an instrumental variable is a randomly assigned treatment for a policy in which the actual treatment still depends on a decision by the agents (or on decisions made by those who execute the program). For instance, long–term unemployed can be randomly assigned to a training program, but for many programs they can still decide not to join, or the training manager can decide to withheld some training from some people. Then, the assignment indicator is an instrument for the actual indicator of training received.

An example that does not use explicit randomization may work for the sanctions studies. In the Netherlands the Unemployment Law does not specify which sanction has to be applied to which offence. So the UI administration offices are free to choose the sanctions they think are appropriate. As a consequence, the application of UI sanctions depend on some extend on the discretion of the employees of the social security councils. Then, if the UI recipients are randomly assigned to different people at the administration offices, the indicator of the civil servant that is responsible for the particular UI recipient can serve as an instrument for the application of a sanction.

In this section we extend the Generalized Accelerated Failure Time (GAFT) Model we introduced in Bijwaard and Ridder (2000) (see also Ridder (1990) and Bijwaard (2001)) to include endogenous covariates. We assume that the time-to-event T is a random variable with an absolutely continuous distribution, for instance the time spent in unemployment of an individual. We concentrate on the studies with selective compliance to randomised treatment assignment. Censoring and time–varying covariates and treatments are easily incorporated. The observed durations may be right–censored, i.e. we observe $\tilde{T} = \min(T, C)$ with C the censoring time. Here we relax the assumption that T and C are conditionally independent given a vector of observable covariates (see section 2.4). The existence of endogenous covariates implies (possible) dependence between the duration and the censoring time.

We assume the intervention can be switched on or off at fixed time points t_k for $k = 0, 1, \dots,$ and $t_0 = 0$. In medical studies the discretized points in time usually coincide with the clinical visits. In the UI sanction studies these fixed points may reflect the monthly check on sufficient applications of the UI recipients. Let D_k denote the treatment status for the time–interval $(t_k, t_{k+1}]$. If $D_k = 1$ the unit is treated in that time interval and, $D_k = 0$ otherwise. Thus, if at a particular visit to the social security council it is decided to cut the benefits of an unemployed individual we assume that sanction will be imposed until, at least, the next visit. We only consider binary treatments, thus in the sanction studies D_k would indicate whether or not a sanction is imposed in the time–interval $(t_k, t_{k+1}]$ and not the amount the benefit is reduced. Before we turn to the formulation of the model we discuss the problems involved in analysing duration data with endogenous covariates.

If everybody enrolled in a randomised trial complied with his or her assigned treatment, standard techniques for the analysis of survival time data can be used to estimate treatment effects. For example, if everyone assigned to a bonus group agreed to participate, we can use a Mixed Proportional Hazards model with conditional hazard

$$\theta(t \mid X, D, V) = V\lambda(t;\alpha_0)e^{\beta_0' X(t)+\gamma_0 D} \tag{1}$$

where time t is measured as time since randomization, $\lambda(t;\alpha_0)$ is the baseline hazard at time t, $X(t)$ is a vector of time–constant and time–varying characteristics of the individual, V is random variable capturing omitted covariates and, D equals one if an individual is in the bonus group and zero otherwise. We can estimate γ_0 using (semi–parametric) Maximum Likelihood procedures, depending on the assumptions we make about the distribution of the unobserved heterogeneity, V, and the baseline hazard. If the model is correctly specified the MLE yields a consistent estimate of the log hazard ratio of receiving a bonus.

However, we will get biased results for the treatment parameters if some of the individuals do not comply with their assigned treatment. The problem is that even if the bonus has no effect on the hazard the treatment parameter γ_0 may not have a causal interpretation, because those who comply with their assigned treatment differ in observed and unobserved characteristics from those who do not comply.

One commonly used solution to this problem, is to ignore the post–randomization compliance and rely on the analysis of the treatment assignment groups. This intention–to–treat analysis replaces the actual treatment by the treatment assignment indicator, R, in (1). Since physical randomization implies that at time zero all attributes of the two treatment groups are (in expectation) identical. Further, if the model is correctly specified the estimated treatment effect will correspond to the overall treatment effect that would be realized in the whole population, under the assumption that the compliance rate and the factors influencing compliance in the sample are identical to those that would occur in the whole population.

The major drawback of the intention–to–treat analysis is that the estimated treatment effect is a mixture of the population effect and the effect on the compliance. Hence, if the bonus effectively raises the re–employment hazard, the intention–to–treat measure of this effect will diminish as non–compliance increases. Another disadvantage is that compliance is very likely to depend on the perceived effects of the treatment. If, for example, the unemployed know that being eligible for a fast re–employment bonus does not stigmatize them, they will be more prone to participate. Thus, when the pattern of compliance is a function of the perceived efficacy of the treatment the estimated intention–to–treat will not represent the overall effect of the treatment would it been adopted in the whole population.

We propose an Instrumental Variable (IV) method for GAFT models that adjusts for the possible endogeneity of the intervention, without suffering the problems of the intention–to–treat method. Conventional IV estimators specialize to Ordinary Least Squares (OLS) in the case where treatment status is an exogenous variable. We develop an estimation procedure that collapses to the Linear Rank Estimator procedure for the GAFT models we proposed in Bijwaard and Ridder (2000) if there is no instrumenting. This is accomplished by using a model that transforms the duration such that for the population treatment effect parameter, the transformed duration is independent of the instrument.

The intuition behind the idea of transforming can be clarified by considering the simple example of an experiment with random assignment and selective compliance at the start of the study. For the moment we assume no other covariates are observed. If the treatment has no impact on the hazards the probability of observing a unit with $R = 1$ among the survivors at some duration t should be equal to the treatment assignment probability at the start. If the treatment has an effect on the hazard this does not hold. However, there always exists a transformation of the duration, $h(T, D; \theta_0)$, depending on the treatment D and a parameter vector θ_0 that make the probabilities equal again, *i.e.*

$$\Pr\big(R = 1 \mid h(T, D; \theta_0) \geq h(t, D; \theta_0)\big) = \Pr\big(R = 1 \mid T \geq 0\big), \tag{2}$$

where θ_0 is the population parameter vector. Note that (2) implies that the hazard of the *population* transformed duration is independent of the instruments.

The transformation model we suggest is closely related to the Rank Preserving Structural Failure Time (RPSFTM) model of Robins and Tsiatis (1991) (see also Mark and Robins (1993b)).[1] This model assumes that the duration an individual would spend in unemployment if never treated does not depend on the treatment arm to which the individual is assigned. This latent baseline duration is related to the observed data through the strong version of the AFT model transformation (see Cox and Oakes (1984)). An important limitation of the RPSFT–model is that it makes a strong non–interaction assumption. This implies that if two units have identical observed survival times and observed treatment histories then they would have had identical survival times had treatment always been withheld. We propose a model that does not impose this non–interaction assumption.

We do not discuss how to identify the Instrumental variable. We assume the data contains a variable which affects the (time–varying) treatment assignment but which does not affect the outcome of interest directly. Abbring and van den Berg (2000) discuss non–parametric identification of treatment effects from non–experimental data that do not need instruments. They use the variation in the timing of the treatment and the outcome variable to identify the treatment effect. They impose an MPH structure on both the treatment process and the outcome process.

[1]The RPSFTM model is a deterministic Structural Nested Failure Time Models (SNFTM) developed by Robins (1992). These models (see also Robins(1989,1993, 1998), Mark and Robins (1993a) and many other publications of Robins) are causal models for the effect of dynamic interventions on a survival time outcome in the presence of time–dependent confounding covariates (but no unmeasured confounding). One estimation procedure for the unknown treatment effect parameter in this model is the method of g–estimation, discussed in Robins (1992)).

2.1 The Generalized Accelerated Failure Time Model

We assume that the time-to-event T is a random variable with an absolutely continuous distribution. The observed durations may be right–censored, i.e. we observe $\tilde{T} = \min(T, C)$ with C the censoring time. The possible time–varying covariates are given by the vector $X_i(t)$ where i refers to a member of the population. The path of the covariates are predetermined. If the path of the intervention is also predetermined the relation between the time-to-event T and the covariates is specified as a GAFT model if[2]

$$\int_0^T \lambda(s;\alpha_0) b\big(X(s);\beta_0\big) e^{\psi(s,D(s);\gamma_0)}\, ds = U_0 \tag{3}$$

where $\lambda(t;\alpha)$ is a non–negative function on $[0,\infty)$. The non–negative regression function $b(.)$ captures the effect of the covariates. $\psi(s, D(s); \gamma_0)$ is the treatment function. We choose parametric but flexible specifications for λ, b and ψ. The impact of the exogenous variables is often taken log linear; $b\big(X(s);\beta_0\big) = \exp\big(\beta_0' X(s)\big)$. A flexible functional form for λ is the piecewise constant function. If we assume that D only changes at fixed durations a flexible functional form of the treatment function is, for example,

$$\psi(s, D(s); \gamma) = \sum_{j=0}^{k} \gamma_j \cdot D_j \cdot I_j(s) \tag{4}$$

with $I_j(s) = I(t_j < s \leq t_{j+1})$.

The GAFT model is characterized by these functions and by the distribution of the non–negative random variable U_0. We denote the c.d.f. of U_0 by G_0 that does not depend on X or D. The last assumption will be relaxed in the next section. In that section we allow the treatment indicator to depend on unobserved random variables that also influence the baseline duration U_0. For the moment we assume the treatment indicator is a variable like any other. The survivor function of U_0 is denoted by $\overline{G}_0$ and its hazard function by μ_0. We assume that the distribution of U_0 is absolutely continuous. The semi–parametric estimators considered in this article avoid assumptions on the distribution of U_0.

We denote the left–hand side of (3) by $h(T, \overline{X}(T), \overline{D}(T); \theta_0)$ with $\theta = (\alpha', \beta', \gamma')'$ the vector of parameters and $\overline{X}(t) = \{X(s); 0 \leq s \leq t\}$. The path of the intervention up to t for $t_k < t \leq t_{k+1}$ is $\overline{D}(t) = \{D_0, D_1, \ldots, D_k\}$. The survivor function of T given t in the GAFT model is

$$\overline{F}(t \mid \overline{X}(t), \overline{D}(t); \theta_0) = \overline{G}_0\Big(h(t, \overline{X}(t), \overline{D}(t); \theta_0)\Big) \tag{5}$$

and the hazard of T in t is

$$\lambda(t;\alpha_0) e^{\beta_0' X(t) + \psi(t, D(t); \gamma_0)} \mu_0\big(h(t, \overline{X}(t), \overline{D}(t); \theta_0)\big) \tag{6}$$

where we used the log–linear assumption on the regression function.

The GAFT model contains as special cases the popular Accelerated Failure Time (AFT), the Proportional Hazard (PH) and the Mixed Proportional Hazard (MPH) models. The AFT model restricts the transformation to $\lambda(t;\alpha_0) \equiv 1$,

[2]In Bijwaard and Ridder (2000) and Bijwaard (2001) we give a more extensive discussion on the GAFT model. Here we repeat the main features of the model.

but leaves the distribution of U_0 unrestricted (with the exception of that U_0 should be non–negative, see e.g. Cox and Oakes (1984)). The (M)PH model restricts the distribution of U_0, but leaves the λ unrestricted (non–negative). The distribution of U_0 is an unit exponential distribution (PH) or a mixture of exponential distributions (MPH).

For a model without covariates and with $\psi(t, D(t); \gamma_0) = \gamma_0 D(t)$ we can interpret the model in terms of the effect of regressing on baseline quantiles. Let $t_q(\overline{D}; \theta_0)$ be the q–th quantile of the population distribution of T with intervention vector D. Let $t_q(\theta_0)$ be the q–th quantile for the reference unit (with $D(t)$ identically equal to zero). Then from (5)

$$\overline{G}_0\Big(h\big(t_q(\overline{D};\theta_0), 0, \overline{D}\big(t_q(\overline{D};\theta_0)\big);\theta_0\big)\Big) = 1 - q = \overline{G}_0\Big(h\big(t_q(\theta_0), 0, 0; \theta_0\big)\Big) \tag{7}$$

Hence we obtain a relation between $t_q(\overline{D}; \theta_0)$ and $t_q(\theta_0)$ that is defined implicitly by

$$\int_0^{t_q(\overline{D};\theta_0)} \lambda(s;\alpha_0) e^{\gamma_0 D(s)}\, ds = \int_0^{t_q(\theta_0)} \lambda(s;\alpha_0)\, ds \tag{8}$$

This implies that

$$\frac{d\, t_q(\overline{D};\theta_0)}{d\, t_q(\theta_0)} = e^{-\gamma_0 D\left(t_q(\overline{D};\theta_0)\right)} \frac{\lambda\big(t_q(\theta_0);\alpha_0\big)}{\lambda\big(t_q(\overline{D};\theta_0);\alpha_0\big)} \tag{9}$$

In the AFT model the time-to-event of a unit with (time–constant) treatment D is distributed as $U_0 e^{-\gamma_0 D}$. The factor $e^{-\gamma_0 D}$ is called the acceleration factor, because the time-to-event for a unit with treatment vector D is obtained by accelerating the time-to-event of the reference unit by this factor. Thus, in the relation between the quantiles in the GAFT model the acceleration factor is multiplied by the ratio of the values of λ in the q–th quantile of the reference and D unit, respectively.

In the MPH model we can interpret λ as the baseline hazard, i.e. the factor in the proportional hazard that captures the (duration) time variation in the hazard function. Thus, in the MPH model the ratio in (9) can be interpreted as the ratio of baseline hazards. The treatment parameter, γ, is the proportional change in the hazard rate due to a unit change in $D(t)$ for a unit with unobserved heterogeneity V.

2.2 GAFT model and endogenous interventions

As we mentioned above, if the treatment is randomly assigned and all units comply with their assigned treatment we can simply add the treatment indicator to the covariates. Then we can apply, for example, the techniques in Bijwaard and Ridder (2000) to estimate the treatment effect. However, if the actual treatment is based on a selective decision we cannot use these methods. In that case, even for the population parameter vector θ_0 the baseline duration U_0 and D are *not* independent. However, if there exist an (dynamic) instrument that is independent of the counterfactual hazard but related to the actual treatment we can develop an instrumental variable procedure.

Consider a (possibly time–dependent) random variable R_k that is constant on $[t_k, t_{k+1})$ and that may change just before the actual treatment, D_k, can change. If this variable is correlated with D_k conditional on the observed history, $\overline{D}_{k-1}$, but independent of the baseline duration U_0, it is an instrument for D_k. We assume the instrument is also binary, just as the treatment indicator. It is not difficult to extend the analysis to more, discrete, levels of both the instrument and the treatment indicator.

We assume that there is no interaction between the units. Thus, knowing your friend who is also unemployed got a sanction for inactive behaviour does not alter your chance on getting a sanction yourself nor the probability to find a job. Further we assume that (i) the instrument has no direct effect on the duration, (ii) the probability of treatment only depends on previous actual treatment and the current treatment assignment and not on previous treatment assignments, (iii) the conditional distribution of the instrument is not degenerate and only depends on the history of the intervention and not of the instrument. The latter two assumptions imply that the instruments are sequentially independent.

An IV method in a ordinary linear model is usually meant for adjusting correlation between the error term and the covariates. The instrumental variable method for estimating the treatment effects in the GAFT models does basically the same. It corrects for the correlation between the treatment and the baseline (transformed) durations. The estimation procedure is based on the independence of R_k and U_0, *i.e.* for $T \geq t > t_k$ and observed covariate and treatment history $\overline{X}(T), \overline{D}_{k-1}$ we require

$$\Pr\Big(R_k = 1 \big| h(T, \overline{X}(T), \overline{D}_k; \theta_0) \geq h(t, \overline{X}(T), \overline{D}_k; \theta_0)\Big) = \\ \Pr\Big(R_k = 1 \big| h(T, \overline{X}(T), \overline{D}_{k-1}; \theta_0) \geq h(t_k, \overline{X}(T), \overline{D}_{k-1}; \theta_0)\Big), \quad (10)$$

It implies that the probability of observing a unit with $R_{ik} = 1$ (conditional on the observed history up to t_k) does not alter over the transformed interval of $(t_k, t_{k+1}]$. This is equivalent to requiring that the hazard rate of the baseline durations and the instruments are independent.

The GAFT model, which implies (3), provides a transformation that fulfills the condition in (10). This independence only holds for the population parameters and therefore we can build an estimation procedure similar to the Linear Rank Estimation of Bijwaard and Ridder (2000) and Bijwaard and Ridder (2002) (see also Bijwaard (2001)) that exploits this. In section 3 we discuss this Instrumental Variable Linear Rank estimator. First, we address the implications of the GAFT model on the treatment effects.

Equation (3) implicitly defines the outcome, *i.e.* duration, distributions for the temporal treatment and control groups. With time–varying interventions and time–varying instruments the relevant distinction is between the group (given the observed treatment history) that has received treatment at some point in time, the temporal treatment group, and the group that has not received the treatment at that point in time, the temporal control group. We indicate these regimes by the subscripts (1) and (0), respectively. If there are no additional covariates x and given $\overline{D}_{k-1}$, the observed treatment history up to t_k, then the outcome distributions for the two regimes for $T^*_{(1)}, T^*_{(0)} > t_k$ are given by[3]

$$U^*_{(0)} = \int_0^{t_k} \lambda(s;\alpha_0) e^{\psi(s,D(s);\gamma_0)} ds + \int_{t_k}^{T^*_{(0)}} \lambda(s;\alpha_0)\, ds \tag{11}$$

$$U^*_{(1)} = \int_0^{t_k} \lambda(s;\alpha_0) e^{\psi(s,D(s);\gamma_0)} ds + \int_{t_k}^{T^*_{(1)}} \lambda(s;\alpha_0) e^{\psi(s,D(s);\gamma_0)} ds \tag{12}$$

The fundamental assumption that assures identification is that $U^*_{(0)}$ and $U^*_{(1)}$ have the same distribution. They may be dependent, but the joint distribution is only identified in special cases and not of particular interest. A special case is that $U^*_{(1)} \equiv U^*_{(0)}$. In that case there is a deterministic relation between the outcomes in the two regimes and,

$$\int_{t_k}^{T^*_{(0)}} \lambda(s;\alpha_0) ds = \int_{t_k}^{T^*_{(1)}} \lambda(s;\alpha_0) e^{\psi(s,D(s);\gamma_0)} ds \tag{13}$$

This model corresponds, but is not identical to the Rank Preserving Structural Failure Time (RPSFTM) model of Robins and Tsiatis (1991). The model is rank preserving. If we consider two units with potential outcomes $T^*_{(0)i}, T^*_{(1)i},\ i = 1, 2,$ then $T^*_{(0)1} > T^*_{(0)2}$ implies $T^*_{(1)1} > T^*_{(1)2}$ and vise versa. However,

$$T^*_{(0)} \neq \int_0^{T^*_{(1)}} \lambda(s;\alpha_0) e^{\psi(s,D(s);\gamma_0)} ds \Big|_{\psi(t,D(t);\gamma_0)\equiv 0,\ \text{for}\ t_k < t \leq t_{k+1}} \tag{14}$$

and $T^*_{(0)}$ cannot be interpreted as the latent baseline failure time. Robins and Tsiatis think of $U^*_{(0)} \equiv U^*_{(1)} = U^*$, with U^* is the baseline duration for the never treated, as a pre–treatment characteristic of the unit. An unattractive feature of this model is that it implies that units with the same outcome in the treatment regime have the same outcome in the no treatment regime, and the other way round. This equality of counterfactuals is a strong assumption.

Under the weaker assumption that $U^*_{(0)}$ and $U^*_{(1)}$ have the same distribution, the relation in (13) still holds but now for the quantiles of the outcome distribution. Thus we can extend the relation between the quantiles of the GAFT model in (9). Let $t_q(0) > t_k$ and $t_q(1) > t_k$ be the quantiles of $U^*_{(0)}$ and $U^*_{(1)}$, both with covariate vector X, then,

$$\frac{dt_q(1)}{dt_q(0)} = e^{-\psi\left(t_q(1),D(t_q(1));\gamma_0\right)} e^{\beta_0'\left(X(t_q(0)) - X(t_q(1))\right)} \frac{\lambda(t_q(0);\alpha_0)}{\lambda(t_q(1);\alpha_0)} \tag{15}$$

If we compare this expression to that given by Robins and Tsiatis (under the stronger assumption $U^*_{(0)} \equiv U^*_{(1)}$, the covariates are time–constant and, $\psi(.,D(.);\gamma_0) = \gamma_0 D$), we see that the third factor, the ratio of the values of λ in the q–th quantile of the temporal control and treatment, is missing. Note that if $D_k(t_q(1)) = 1,\ \psi(t_q(1), D_k(t_q(1));\gamma_0) = 0$ and $\lambda(t_q(0);\alpha_0) < \lambda(t_q(1);\alpha_0)$, the RPSFT model concludes that the treatment effect is positive. Hence failure to correct for variation in the baseline intensity may bias the estimate of the treatment effect.

[3]To ease exposure we assume that the treatment function is zero on the relevant interval if $D_k = 0$. The model can easily incorporate more general treatment functions.

The hazard or re–employment rate of unemployment durations often exhibits a spike just before the time that unemployment benefits are exhausted. This corresponds to a large increase in λ at that spike. For the bonus data this implies that if we assume that all unemployed who find a job receive the bonus and that the bonus has a small positive effect on the job finding rate, so that $t_q(0) > t_q(1)$. If $t_q(0)$ is in the spike while $t_q(1)$ is not, then the left–hand side of (15) is greater than one and the AFT treatment effect at $t_q(1)$ is negative. Thus if there is (substantial) variation in λ, the AFT treatment effects will be biased.

Note that the transformation in Bijwaard and Ridder (2002),

$$U = b \int_0^T \lambda(s) e^{\gamma D(s)} \, ds \equiv h(T) \tag{16}$$

is embedded in transformation of the durations presented here. An important difference with Bijwaard and Ridder (2002) is that here we do not assume an MPH model. Our proposed Instrumental Variable Linear Rank estimator leaves the distribution of U_0 unspecified, because it does not require assumptions on this distribution. This is a major advantage, because inference on the distribution of the unobserved heterogeneity in a MPH model is unreliable.

We briefly mention some identification problems. In Bijwaard and Ridder (2000) we have shown that in the GAFT model without endogenous covariates identification depends on whether the covariates are time constant or time–varying. If the covariates are time constant we can identify the transformation Λ, the integral of λ, up to a power and β up to scale (with the power and the scale being equal).[4]

If the interventions and the treatment function are also time constant and the treatment function is linear in γ all observationally equivalent GAFT models, i.e. models that give the same conditional distribution of T given X and D, have regression parameters $d\beta_0$, treatment parameters $d\gamma$, integrated transformation $c\Lambda(t;\alpha_0)^d$ and U_0 distribution $G_0\left(\left(\frac{u}{c}\right)^{1/d}\right)$ for some constants $c, d > 0$.

If the covariates and/or the treatments are time–varying we can identify Λ and the distribution of U_0 up to a common scale parameter. Note that we can also reduce the set of observationally equivalent GAFT models by taking a time–varying treatment function. For example, in the Illinois re–employment Bonus experiment the unemployed know in advance that the treatment, getting a \$500 bonus for finding a job, lasts only for a limited period of the time they receive unemployment benefits. Those who choose to participate can only obtain the bonus if they find a job within 10 weeks. We have two equivalent ways of specifying the treatment function, or we let the function change after 10 weeks or we keep the function time constant and let the treatment change after 10 weeks.

Because we leave U_0 unspecified in our estimation method, we can not use restrictions on U_0 to find the scale parameter. For that reason we normalize $\Lambda(t;\alpha_0)$ by setting $\Lambda(t_0;\alpha_0) = 1$ for some $t_0 > 0$. With time constant regressors and treatments we need the same normalisation, but in addition we need to set

[4] The restrictions on the baseline hazard for the MPH model as introduced by Ridder and Woutersen (2001) to secure non-singularity of the efficiency bound are related.

one regression coefficient equal to one. Of course, we could choose a class of transformations that is not closed under the power transformation. This amounts to identification by functional form. Finally, we need that $\ln\lambda$, D and X are not collinear.

2.3 Counting process interpretation

The c.d.f. and p.d.f. of the distribution of a duration T can be expressed as functions of the hazard rate. These expressions can be used to obtain a likelihood function. We use a different (but of course equivalent) representation of the relation between the hazard rate and the random duration. In particular, we use the framework of counting processes (see e.g. Andersen, Borgan, Gill, and Keiding (1993) and Klein and Moeschberger (1997)). The main advantage of this framework is that it allows us to express the duration distribution as a regression model with an error term that is a martingale difference. This simplifies the analysis of estimators. The conditions for non selective observation can be precisely stated in this framework. The same is true for conditions on time–varying covariates.

The starting point is that the hazard of T is the intensity of the counting process $\{N(t); t \geq 0\}$ that counts the number of times that the event occurs during $[0, t]$. The counting process has a jump $+1$ at the time of occurrence of the event[5]. A jump occurs if and only if $dN(t) = N(t) - N(t-) = 1$. For duration data, the event can only occur once. In many unemployment studies the unit are only observed until re-employment. So, at most one jump is observed for any unit. To account for this we introduce the observation indicator $Y(t) = I(T \geq t)$ that is zero after re–employment. By specifying the intensity as the product of this observation indicator and the hazard rate we effectively limit the number of occurrences of the event to one. We assume that the observation indicator only depends on events up to time t. The observation process is assumed to have left–continuous sample paths. We define the history of the process up to time t by $H(t) = \{\overline{Y}(t), \overline{D}(t), \overline{X}(t)\}$, where $\overline{Y}(t) = \{Y(s), 0 \leq s \leq t\}$. The history $H(t)$ only contains observable events.

Let $\overline{V}(t)$ be the path of some unobserved (possibly time–varying) variables up to t that both influence the treatment decision and the time–to–event. An example is the, usually, unobserved search intensity of unemployed looking for a job. We assume that $\overline{V}(t)$ and $\overline{X}(t)$ are stochastically independent. Denote $H^V(t) = \{H(t), \overline{V}(t)\}$, the history that also includes the path of the unobservables. As with dynamic regressors in time-series models, the time-varying $D(t)$ and $X(t)$ may depend on the dependent variable up to time t but not after time t (conditionally on $V(t)$). Thus $D(t)$ only depends on $H^V(t)$ and $X(t)$ only on $H(t)$. In the counting process literature such a time-varying covariate is called predictable. We will use the econometric term predetermined.

If the conditional distributions of $N(t)$ given either $H^V(t)$ or $H(t)$ are well-defined (see Andersen, Borgan, Gill, and Keiding (1993) for assumptions that ensure this) we can express the probability of an event in $(t - dt, t]$ as[6]

$$\Pr(dN(t) = 1 \mid H^V(t)) = Y(t)\kappa(t \mid \overline{X}(t), \overline{D}(t), \overline{V}(t))\, dt \tag{17}$$

[5] The sample paths are assumed to be right-continuous.

[6] Because the sample paths of $\{Y(t), X(t), t \geq 0\}$ are assumed to be left-continuous (as is the baseline hazard), we can to substitute t for $t - dt$ in (17).

with $\kappa(t \mid \cdot)$ is the hazard of T at t given $\overline{X}(t), \overline{D}(t)$ and $\overline{V}(t)$. By the Doob-Meier decomposition

$$dN(t) = Y(t)\kappa(t \mid \overline{X}(t), \overline{D}(t), \overline{V}(t))\, dt + dM(t) \tag{18}$$

with $\{M(t); t \geq 0\}$ a (local square integrable) martingale. The conditional mean and variance of this martingale are

$$\begin{aligned} \mathrm{E}\big(dM(t) \mid H(t)\big) &= 0 && (19) \\ \mathrm{Var}\big(dM(t) \mid H(t)\big) &= Y(t)\kappa(t \mid \overline{X}(t), \overline{D}(t), \overline{V}(t))\, dt && (20) \end{aligned}$$

The (conditional on $H(t)$) mean and variance of the counting process are equal, so that the disturbances in equation (18) are heteroscedastic. The probability in equation (17) is zero, if the unit is not under observation.

A counting process can be considered as a sequence of Bernoulli experiments, because if dt is small equations (17) and (20) give the mean and variance of a Bernoulli random variable. The relation between the counting process and the sequence of Bernoulli experiments is given in equation (18), that can be considered as a regression model with an additive error that is a martingale difference. This equation resembles a time-series regression model. The Doob-Meier decomposition is the key to the derivation of the distribution of the estimators, because the asymptotic behavior of partial sums of martingales is well-known.

The GAFT model is defined in (3) as a transformation of the observed time-to-event T to a baseline duration U_0. The transformation involved a parameter vector θ_0. We denote the transformation for parameter vectors $\theta \neq \theta_0$ by $U(\theta)$ with $U_0 = U(\theta_0)$. Just as the distribution of T, that of $U(\theta)$ can be represented by a (transformed) counting process $\{N^U(u); u \geq 0\}$. The relation between the original and transformed counting process, observation indicator, time-varying covariate and, time-varying treatment is

$$\begin{aligned} N^U(u) &= N\big(h^{-1}(u;\theta)\big) \qquad & Y^U(u) &= Y\big(h^{-1}(u;\theta)\big) \\ X^U(u) &= X\big(h^{-1}(u;\theta)\big) \qquad & D^U(u) &= D\big(h^{-1}(u;\theta)\big) \end{aligned}$$

with $h(T;\theta) = h(T, \overline{X}(T), \overline{D}(T); \theta)$. For $\theta = \theta_0$ we denote $h_0(T) = h(T, \overline{X}(T), \overline{D}(T); \theta_0)$. The corresponding history is $H^U(u;\theta) = \{\overline{Y}^U(u;\theta), \overline{X}^U(u;\theta), \overline{D}^U(u;\theta)\}$. The intensity of the transformed counting process with respect to history $H^U(u;\theta)$ is (see Andersen, Borgan, Gill, and Keiding (1993), p. 87)[7]

$$\begin{aligned} \Pr\big(dN^U(u;\theta) = 1 \mid H^U(u;\theta)\big) = Y^U(u;\theta)\mathrm{E}\Bigg[& \frac{\lambda\big(h^{-1}(u;\theta);\alpha_0\big)}{\lambda\big(h^{-1}(u;\theta);\alpha\big)} e^{(\beta_0-\beta)'X^U(u;\theta)} \\ & \times \exp\Big(\psi(h^{-1}(u;\theta), D^U(u;\theta), \gamma_0) - \psi(h^{-1}(u;\theta), D^U(u;\theta), \gamma)\Big) \\ & \times \mu_0\Big(h_0\big(h^{-1}(u;\theta)\big)\Big) \Bigg| H^U(u;\theta)\Bigg]\, du \end{aligned} \tag{21}$$

[7] If $U = h(T)$ and λ_T is the hazard rate of the distribution of T, then the hazard rate of the distribution of U is

$$\lambda_U(u) = \lambda_T(h^{-1}(u))\frac{1}{h'(h^{-1}(u))}$$

We implicitly integrate with respect to the distribution of the unobserved $V(t)$ conditional on $H^U(u)$. Note that these unobserved covariates are only introduced to ascertain the predictability of the treatment process. Although the distribution of those variables determines the distribution of U_0, the consistency of the IVLR is independent of that distribution.[8] Unfortunately, even for the population parameters θ_0 the hazard of U_0, $\mu_0(u)$, still depends on the intervention path (through the correlation with $V(.)$). If we condition on the history of the instruments instead of the actual treatment we do get the independence.

If we consider (transformed) durations from a (sequentially) randomized experiment, we must add the (path of the) randomization indicator R to the conditioning variables in (21). Let the UR–history, $H^{UR}(u) = \{Y^U(s), X^U(s), R^U(s); 0 \leq s \leq u\}$, be the history on the transformed durations in which $\overline{D}^U(u)$ is replaced by the history of the randomization indicator on the transformed time, $R^U(u) = R\big(h^{-1}(u;\theta)\big)$. Then, another application of the innovation theorem gives the intensity of the transformed process on the UR–history

$$\begin{aligned}\Pr\big(dN^U(u;\theta) = 1 \mid H^U(u;\theta)\big) &= Y^U(u;\theta)\mathrm{E}\Bigg[\frac{\lambda\big(h^{-1}(u;\theta);\alpha_0\big)}{\lambda\big(h^{-1}(u;\theta);\alpha\big)}e^{(\beta_0-\beta)'X^U(u;\theta)} \\ &\times \exp\Big(\psi(h^{-1}(u;\theta), D^U(u;\theta), \gamma_0) - \psi(h^{-1}(u;\theta), D^U(u;\theta), \gamma)\Big) \\ &\times \mu_0\Big(h_0\big(h^{-1}(u;\theta)\big)\Big)\Bigg| H^{UR}(u;\theta)\Bigg]\, du \qquad (22)\end{aligned}$$

which for the population parameters simplifies to $Y_0^U(u)\mu_0(u)du$ with $Y_0^U(u) = Y^U(u;\theta_0)$ and $H_0^{UR}(u) = H^{UR}(u;\theta_0)$. Note that (21) and (22) only differ in the history the intensities are conditioned on.

The intensity in (22) is independent of (the history) of X and R if we substitute the population parameter values, but not for other values of the parameters. This result is the basis for identification of the parameters. Independence of (the history) of X and R and the hazard rate of U_0 implies that the quantiles of the distribution of U_0 do not depend on X or R. By choosing C_0^U such that $\Pr(U_0 \leq C_0^U) = q$ we restrict the independence to the quantiles up to the q–th. For further reference we denote the intensity in (22) by $\mu_i^U(u;\theta)$ which reduces to $\mu_0(u)$ for the population parameters.

2.4 Censoring and endogenous treatment

A common feature of time–to–event data is that some of the observations are censored. Assume the censoring time, C, is (potentially) known at each point in time a change of treatment can occur. As in the bonus experiment this censoring time usually corresponds to the end of the observation period. Thus, at each duration t_k we know the potential censoring times for all units still at risk (e.g. we know the end of the observation period). Then, the observed variables are $\tilde{T} = \min(T, C)$ and $\Delta = I(T \leq C)$, where Δ is one if T is observed.

One is tempted to assume we can define the censored transformed durations by $\tilde{U}(\theta) = \min\big(h(T;\theta), h(C;\theta)\big) = h(\tilde{T};\theta)$. However, with endogenous covariates censoring makes some of the orthogonality conditions fail to hold. This

[8] In Bijwaard and Ridder (2002) we discussed the special case when $V(t) = V(0)$ enters the hazard rate multiplicatively. Here we do not make any assumptions on how $V(t)$ enters the hazard.

can be illustrated by a simple example: Consider, as we observe in the bonus data, a fixed censoring time. Then for all units, irrespective of treatment regime, censoring occurs at time C. Let the (endogenous) intervention, D, and other covariates all be determined at the start of the study and have a constant effect on the hazard. Finally, we assume that except for the treatment parameter, all parameters, β_0 and α_0, are known. Then, the transformation is

$$U_0 = e^{\gamma_0 D + \beta_0' X} \Lambda_0(T) \tag{23}$$

with Λ the integral of λ. Hence, if $D = 0$ the censoring in the transformed time is at $e^{\beta_0' X} \Lambda_0(C)$, but if $D = 1$ the censoring time is $e^{\beta_0' X + \gamma_0} \Lambda_0(C)$. Thus, if $\gamma_0 > 0$, then all transformed durations in the interval $[e^{\beta_0' X} \Lambda_0(C), e^{\beta_0' X + \gamma_0} \Lambda_0(C)]$ have $D = 1$, *i.e.* belong to the treatment group (for $\gamma_0 < 0$ the boundaries are reversed). The hazard of U_0 on this interval is clearly not independent of D and hence of R. The independence of the hazard of U_0 and R only holds up to the lower bound of the interval. This implies that in the IVLR, which exploits this independence, the transformed durations that fall in the problematic interval have to be censored.

This can be generalized to the model with time–varying interventions and covariates. We denote, for $t_k < t \leq t_{k+1}$, the treatment function at t if $D_k = 1$ by $\psi_k^1(t;\gamma)$ and if $D_k = 0$ by $\psi_k^0(t;\gamma)$. Simple examples of these treatment functions are $\psi_k^1(t;\gamma) = e^{\gamma_k}$ and $\psi_k^0(t) = 1$. We define the transformed censoring time $C^U(\theta)$ (possibly depending on the observed history of other covariates) such that: (a) $T \geq C$ implies $h(T;\theta) \geq C^U(\theta)$ and (b) U_0 and R_k are independent on the interval bounded above by $C^U(\theta)$.

Note that we either observe $T \leq C$ and $\Delta = 1$, or $T > C$ and $\Delta = 0$. If some of the other covariates are also time–varying we have another identification problem, because these covariates are only observed up until $\tilde{T}$. The transformed censoring times (conditional on $T, C > t_k$) that take all these considerations into account are the sum of the transformed duration up to t_k, $h(t_k;\theta)$ and the censoring adjustment, *i.e*

$$\begin{aligned} C^U(\theta) = & \int_0^{t_k} \lambda(s;\alpha) e^{\beta' X(s) + \psi(s, D(s);\gamma)} ds \quad + \\ & + \begin{cases} \int_{t_k}^{C} \lambda(s;\alpha) e^{\beta' X(s)} \min(\psi_k^1(s;\gamma), \psi_k^0(s;\gamma))\, ds & \text{if } T > C, \\ \int_{t_k}^{T} \lambda(s;\alpha) e^{\beta' X(s)} \min(\psi_k^1(s;\gamma), \psi_k^0(s;\gamma))\, ds + \int_T^C \lambda(s;\alpha)\, ds & \text{if } T \leq C. \end{cases} \end{aligned} \tag{24}$$

From the last term on the right–hand side of (24) we see why we need to know C even for the uncensored observations. Otherwise we can not compute $C^U(\theta)$ for these observations. We can estimate the parameters of the Instrumental GAFT model from the following observed data

$$\tilde{U}(\theta) = \min\big(U(\theta), C^U(\theta)\big), \qquad \Delta^U(\theta) = I\big(U(\theta) < C^U(\theta)\big)$$

and $Y^U(u;\theta) = I\big(\tilde{U}(\theta) \geq u\big)$. Now $\tilde{U}(\theta_0)$ is independent of R_k for $\Delta^U(\theta_0) = 1$. Note that if both $\psi_k^1(t;\gamma)$ and $\psi_k^0(t;\gamma)$ differ from one, we get extra censoring on the transformed durations, because some units with $\Delta = 1$ have $\Delta^U(\theta) = 0$. In the next section we use the independence of $\{\tilde{U}(\theta_0), \Delta^U(\theta_0)\}$ and R_k to obtain estimates of the parameters of the model.

3 Instrumental Variable Linear Rank Estimation of the treatment Effect

In this section we introduce the Instrumental Variable Linear Rank (IVLR) estimator of the GAFT model with instruments. The estimator is based on the transformed durations $U(\theta)$ and the properties of the IVLR are derived using the transformed counting process $N^U(u)$. The IVLR is motivated by (22) and is defined on the, possibly censored durations $\tilde{U}(\theta)$.

3.1 The IVLR estimator

For the population parameter vector θ_0 the hazard of U_0, $\mu_0(u)$, is independent of the covariate and instrument history up to $h_0^{-1}(u)$. Because this is true only for $\theta = \theta_0$, we can use an estimate of (22) as an estimating equation. This independence can be used to construct test statistics close to the linear rank test (see Prentice (1978)). The IVLR also exploits this independence and is the estimation procedure derived from these rank tests.

In Bijwaard and Ridder (2002) we suggested a Two–Stage Linear Rank Estimator (2SLR) for the (M)PH models, a submodel of the GAFT models. The 2SLR is partially an Instrumental Variable method. In the first stage of the 2SLR the regression parameters and the parameters of the baseline hazard are obtained by Maximum Likelihood Estimation on the control group data. These estimated parameters are substituted in (3) to compute the transformed durations. These transformed duration are also a function of the treatment parameter which is estimated in the second stage by a Instrumental Variable LRE.

The 2SLR estimator requires preliminary estimates of the regression and baseline hazard parameters. However, only if there is full compliance in the control group it is possible to get these first stage estimators. The (M)PH assumption of the 2SLR is also questionable; it restricts the distribution of U_0 to a mixture of exponentials. In this section we suggest a one stage Instrumental Variable LRE that allows to estimate all the parameters of the GAFT model, the regression parameters, the parameters of λ and the treatment parameters, simultaneously without assuming full compliance in the control group (or temporal control groups) nor assuming an (M)PH model.

We mention again that the IVLR–estimator is also closely related to the Rank Preserving Structural Failure Time Model (RPSFTM) estimator in Robins and Tsiatis (1991). As we argued before the GAFT models are not rank preserving as the RPSFTM and, therefore, do not impose the non–interaction assumption of the RPSFT-models.

The estimating equation that defines the IVLR estimator contains a a left–continuous weight function W. The dimension of W is greater than or equal to the dimension of θ_0 that is p. The weight function may depend on $\overline{X}_i^U(u;\theta)$ and $\overline{R}_i^U(u;\theta)$. Typical examples are $\overline{X}^U(u;\theta)$ and $R_i^U(u;\theta)$. The variance of the IVLR estimator depends on W and in section 3.2 we discuss the optimal choice of this function. The IVLR estimator is defined by the estimating equations

$$S_n(\theta;W)=\sum_{i=1}^{n}\Delta_i^U(\theta)\left\{W\big(\tilde U_i(\theta),\overline{X}_i^U\big(\tilde U_i(\theta);\theta\big),\overline{R}_i^U\big(\tilde U_i(\theta);\theta\big)\big)-\overline{W}(\tilde U_i(\theta);\theta)\right\} \tag{25}$$

where

$$\overline{W}(\tilde U_i(\theta);\theta)=\frac{\sum_{j=1}^n Y_j^U(\tilde U_i(\theta);\theta)W\big(\tilde U_i(\theta),\overline{X}_j^U\big(\tilde U_i(\theta);\theta\big),\overline{R}_j^U\big(\tilde U_i(\theta);\theta\big)\big)}{\sum_{j=1}^n Y_j^U(\tilde U_i)},$$

the average of the weight function evaluated at $\tilde U_i(\theta)$ among the individuals still at risk. Note that we used $\Delta_i^U(\theta)$ instead of Δ_i to assure independence of the instruments and the transformed durations for all uncensored observations.

The statistic $S_n(\theta;W)$ has mean zero in the population parameters and, therefore, we base our estimator on the roots of $S_n(\theta;W)=0$. However, the estimating functions are discontinuous, piecewise constant, functions of θ and a solution may not exist. For that reason we define the Instrumental Linear Rank estimator (IVLR) $\hat\theta_n(W)$ as the minimizer of the quadratic form, *i.e.*

$$\hat\theta_n(W)=\inf\{\theta\mid S_n(\theta;W)'S_n(\theta;W)\} \tag{26}$$

The counting process interpretation of duration models allows for another, of course equivalent, formulation of the estimating equations in (25). The relevant counting measure, $N_i^U(u;\theta)$, can be seen as a discrete 'probability distribution' that assigns weight unity to uncensored transformed durations and is zero elsewhere. Then the estimating equations can be expressed as an integral with respect to that counting process

$$S_n(\theta;W)=\sum_{i=1}^n\int_0^{C_i^U(\theta)}\left\{W\big(u,\overline{X}_i^U(u;\theta),\overline{R}_i^U(u;\theta)\big)-\overline{W}(u;\theta)\right\}dN_i^{\tilde U}(u;\theta) \tag{27}$$

where $C_i^U(\theta)$ is the transformed censoring time defined in (24). To ensure weak consistency and asymptotic normality of the IVLR estimator we make the following assumptions. The intervention can be switched on or off at fixed time points t_k for $k=0,1,\dots,$ and $t_0=0$. Let D_k denote the treatment status for the time–interval $(t_k,t_{k+1}]$. If $D_k=1$ the unit is treated in that time interval and, $D_k=0$ otherwise. The, possibly time–dependent, random variable R_k is an instrument that is constant on $[t_k,t_{k+1})$ and may change just before the actual treatment, D_k, can change. We restrict both the instrument, R_k, and the treatment, D_k, to be binary.

C1: The covariate process $X(t)$ is predetermined, i.e. its distribution is independent of $\{H(s),s>t\}$. The sample paths of the covariate process are bounded and at least one of time–varying covariates is a continuous variable.

C2: The observation process $Y(t)$ is caglad and $Y(t)$ is predetermined. Moreover,

$$\Pr\big(dN(t)=1\mid Y(t)=1,H(t)\big)=\Pr\big(dN(t)=0\mid Y(t)=0,H(t)\big)$$

C3: The population distribution of T given $\overline{X}$ and $\overline{D}$ satisfies

$$\int_0^T \lambda(s;\alpha_0)e^{\beta_0' X(s)+\psi(s,D;\gamma_0)}\, ds = U_0$$

The absolutely continuous distribution of U_0 does not depend on $\overline{X}$ or $\overline{R}$. The p.d.f. of U_0 is bounded.

C4: The transformed observation process $Y^U(u;\theta) = I\big(\tilde{U}(\theta) \geq u\big)$ is caglad and predetermined, with $\tilde{U}(\theta) = \min\big(U(\theta), C^U(\theta)\big)$ and $C^U(\theta)$ defined in (24).

C5: The instrumental function W is bounded and left–continuous.

C6: The intensity of $U(\theta)$, $\mu_i^U(u;\theta)$ given history $H^{UR}(u;\theta)$ in (22) can be linearised in a neighbourhood of θ_0 as a function of θ, *i.e.* there exist $\kappa(u)$ and $\epsilon > 0$ such that for $\|\theta-\theta_0\| < \epsilon$

$$\left| \mu_i^U(u;\theta) - \mu_0(u) - (\theta-\theta_0)' \frac{\partial \mu_i^U(u;\theta)}{\partial\theta}\bigg|_{\theta=\theta_0} \right| \leq \|\theta-\theta_0\|^2 \kappa(u)$$

for $u \leq C^U(\theta_0)$.

C7: There exists a continuous function $a(u;\theta)$ of θ in a neighbourhood B of θ_0 such that

$$\sup_{u\leq C^U(\theta_0)} \sup_{\theta\in B} \left\| \overline{W}(u;\theta) - a(u;\theta) \right\| \xrightarrow{p} 0$$

where

$$\overline{W}(u;\theta) = \frac{\sum_{j=1}^n Y_j^U(u;\theta) W\big(u, \overline{X}_j^U(u;\theta), \overline{R}_j^U(u;\theta)\big)}{\sum_{j=1}^n Y_j^U(u;\theta)}$$

C8: There exists a continuous matrix function $A(u;\theta)$ of θ in a neighbourhood B of θ_0 such that

$$\sup_{u\leq C^U(\theta_0)} \sup_{\theta\in B} \Bigg\| \frac{1}{n}\sum_{i=1}^n \Big[W\big(u, \overline{X}_i^U(u;\theta), \overline{R}_i^U(u;\theta)\big) - \overline{W}(u;\theta) \Big] \\ \times \Big[W\big(u, \overline{X}_i^U(u;\theta), \overline{R}_i^U(u;\theta)\big) - \overline{W}(u;\theta) \Big]' Y_i^U(u;\theta) - A(u;\theta) \Bigg\| \xrightarrow{p} 0$$

C9: There exists a continuous matrix-function $Z(u;\theta)$ of θ in a neighbourhood B of θ_0 such that

$$\sup_{u\leq C^U(\theta_0)} \sup_{\theta\in B} \Bigg\| \frac{1}{n}\sum_{i=1}^n \Big[W\big(u, \overline{X}_i^U(u;\theta), \overline{R}_i^U(u;\theta)\big) - \overline{W}(u;\theta) \Big] d_{i0}(u)' Y_i^U(u;\theta) - \\ - Z(u;\theta) \Bigg\| \xrightarrow{p} 0$$

with

$$d_{i0}(u) = \frac{\partial \mu_i^U(u;\theta)}{\partial\theta}\bigg|_{\theta=\theta_0}$$

As for the 2SLR and the LRE the discontinuities of the estimating equation complicate the development of the asymptotic theory of the IVLR estimator. If $S_n(\theta; W)$ were differentiable with respect to θ, then asymptotic normality can be proved using Taylor series expansion in a neighbourhood of θ_0. Tsiatis (1990) showed that, if $S_n(\theta; W)$ is not differentiable, as in the current problem, we can still use a linear approximation of $n^{-1/2} S_n(\theta; W)$. Using this approximation and the asymptotic normality of $S_n(\theta_0; W)$, we can show that $\sqrt{n}(\hat{\theta}_n(W) - \theta_0)$ is asymptotically normal.

The asymptotic properties of the 2SLR in Bijwaard and Ridder (2002) and of the LRE in Bijwaard and Ridder (2000) are based on a similar linearization. One difference is that in the IVLR the weight functions depend on the, transformed, instruments but not on the actual treatment. A second difference is that due to the possible endogeneity of the treatment, the transformed durations are restricted to the interval bounded above by the transformed censoring time (see section 2.4).

This amounts to a small modification of the proof of the asymptotic linearity of $S_n(\theta; W)$ in the neighbourhood of the population parameter θ_0 as given in Bijwaard and Ridder (2000).

$$\begin{aligned} S_n(\theta; W) &= \sum_{i=1}^{n} \int_0^{C_i^U(\theta_0)} \Big\{ W\big(u, \overline{X}_i^U(u;\theta), \overline{R}_i^U(u;\theta)\big) - \overline{W}(u;\theta) \Big\}\, dN_i^U(u;\theta) \\ &\quad + \sum_{i=1}^{n} \int_{C_i^U(\theta)}^{C_i^U(\theta_0)} \Big\{ W\big(u, \overline{X}_i^U(u;\theta), \overline{R}_i^U(u;\theta)\big) - \overline{W}(u;\theta) \Big\}\, dN_i^U(u;\theta) \end{aligned} \tag{28}$$

Substitution of the Doob–Meier composition in the first term on the right for N_i^U gives

$$\begin{aligned} S_n(\theta; W) &= \sum_{i=1}^{n} \int_0^{C_i^U(\theta_0)} \Big\{ W\big(u, \overline{X}_i^U(u;\theta), \overline{R}_i^U(u;\theta)\big) - \overline{W}(u;\theta) \Big\}\, dM_i^U(u;\theta) \\ &+ \sum_{i=1}^{n} \int_0^{C_i^U(\theta_0)} \Big\{ W\big(u, \overline{X}_i^U(u;\theta), \overline{R}_i^U(u;\theta)\big) - \overline{W}(u;\theta) \Big\}\, \mu_i^U(u;\theta) Y_i^U(u;\theta)\, du \end{aligned} \tag{29}$$

We consider both terms separately. The first term is for θ close to θ_0 close to $S_n(\theta_0; W)$ and for the second term we have

$$\begin{aligned} &\sum_{i=1}^{n} \int_0^{C_i^U(\theta_0)} \Big\{ W\big(u, \overline{X}_i^U(u;\theta), \overline{R}_i^U(u;\theta)\big) - \overline{W}(u;\theta) \Big\} \frac{\partial \mu_i^U(u;\theta)'}{\partial \theta} Y_i^U(u;\theta)\, du \\ &\qquad \times (\theta - \theta_0) + O_p\big(\|\theta - \theta_0\|^2\big) \end{aligned}$$

Returning to (28) we note that the second term in this equation equals

$$\begin{aligned} &\sum_{i=1}^{n} \Bigg\{ \Big[W\Big(C_i^u(\theta_0), \overline{X}_i\big(C_i^u(\theta_0);\theta_0\big), \overline{R}_i\big(C_i^u(\theta_0);\theta_0\big)\Big) - \overline{W}\big(C_i^u(\theta_0);\theta_0\big) \Big] \\ &\qquad \times \theta_0\big(C_i^u(\theta_0)\big) Y_i\big(C_i^u(\theta_0);\theta_0\big) \Bigg\} + O_p\big(\|\theta - \theta_0\|^2\big) \end{aligned}$$

The term between brackets is the covariance between $\theta_0\big(C_i^u(\theta_0)\big)$ and $W\big(C_i^u(\theta_0), \overline{X}_i(C_i^u(\theta_0);\theta_0), \overline{R}_i(C_i^u(\theta_0);\theta_0)\big)$ which is zero. Thus this whole term is zero for θ close to θ_0 and we have

$$S_n(\theta;W) \approx S_n(\theta_0;W) + n\int_0^{C^U(\theta_0)} Z(u;\theta_0)\,du \cdot (\theta-\theta_0) \tag{30}$$

Hence, approximately for the IVLR estimator $\hat{\theta}_n(W)$

$$\sqrt{n}(\hat{\theta}_n(W)-\theta_0) = \left[\int_0^{C^U(\theta_0)} Z(u;\theta_0)\,du\right]^{-1} \frac{1}{\sqrt{n}} S_n(\theta_0;W) \tag{31}$$

The asymptotic properties of the IVLR estimator are summarized in the following two theorems.

Theorem 1 (Consistency).
If conditions C1 to C7 hold $\hat{\theta}_n(W)$ converges in probability to θ_0.

Proof: See the appendix.

Theorem 2 (Asymptotic Normality).
If conditions C1 to C9 hold and $Q(W)$ has full rank, then

$$\sqrt{n}(\hat{\theta}_n(W)-\theta_0) \xrightarrow{d} N\big(0, Q^{-1}(W)\Omega(W)Q'^{-1}(W)\big) \tag{32}$$

where

$$\Omega(W) = \int_0^{C^U(\theta_0)} a(u;\theta_0)\mu_0(u)\,du \tag{33}$$

the asymptotic variance of $n^{-1/2}S_n(\theta_0;W)$ and,

$$Q(W) = \int_0^{C^U(\theta_0)} Z(u,\theta_0)\,du \tag{34}$$

the limiting covariance matrix of the processes $W(u, \overline{X}_{i0}^U(u), \overline{R}_{i0}^U(u))$ and $d_{i0}(u)/\mu_0(u)$.

Proof: See the appendix.

3.2 Efficiency

Many different choices of the weight function lead to consistent estimates of the parameters. By properly choosing the weight function the asymptotic variance of the IVLR can be minimized. The optimal weight function of the IVLR is very similar to the optimal weight function we derived in Bijwaard and Ridder (2000) for LRE of the GAFT model without instrumenting.

Theorem 3 (Optimal weight function in IVLR).
The W–function that gives the smallest asymptotic variance for $\hat{\theta}_n(W)$ is

$$W_{\text{opt}}(u, \overline{X}(u), \overline{R}(u)) \propto \left.\frac{\partial \ln \mu^U(u;\theta)}{\partial\theta}\right|_{\theta=\theta_0} = \frac{1}{\mu_0(u)}\frac{\partial\mu^U(u;\theta_0)}{\partial\theta} \tag{35}$$

Note that the only difference between the optimal weight function for the IVLR and the LRE of Bijwaard and Ridder (2000) is that for the IVLR estimator we take the derivative of the hazard conditional on the path of the instruments instead of the path of the actual treatment. Therefore, the argument for the proof of this theorem is almost identical to the proof of the optimal W–function for the Linear Rank Estimator (LRE) without instrumenting. The latter can be found in Bijwaard and Ridder (2000). Hence, this proof is not repeated here.

Again, the asymptotic covariance matrix of the optimal IVLR estimator reduces to

$$\Omega^{-1}(W_{\text{opt}}) = Q^{-1}(W_{\text{opt}}). \tag{36}$$

which is the analog of the asymptotic variance matrix of the optimal LRE.

Despite the simple formulation in theorem 3, these optimal W–function can be very complicated. The next example, which is based on the model we use in the empirical application, shows that in simple GAFT model the optimal weight functions are already troublesome to grasp.

Example 1 *(Optimal weight function for piecewise constant λ and treatment effect).* Consider[9]

$$\lambda(t;\alpha) = \sum_{k=1}^{K-1} e^{\alpha_k} I_k(t) \tag{37}$$

where $I_k(t) = I(t_{k-1} \leq t < t_k)$ and $t_0 = 0, t_K = \infty$. $X(t)$ is a vector of predetermined covariates. We assume the treatment effect is also piecewise constant on, possibly, other intervals than λ, *i.e.*,

$$\psi(t, D; \gamma) = D \sum_{l=1}^{L} \gamma_l \cdot I_l(t) \tag{38}$$

where the actual treatment D is chosen at the start, but the effect may change at fixed durations (e.g. after 10 weeks in the bonus experiment). For each $k = 1, \ldots, K-1$ we define $I_k^D(u) = I\big(m_{k-1}(\overline{X}, D) \leq u < m_k(\overline{X}, D)\big)$ with $m_k(\overline{X}, D) = \int_0^{t_k} \lambda(s;\alpha) e^{\psi(s,D;\gamma)+\beta' X(s)}\, ds$ and similar for $l = 1, \ldots, L$.

Obvious choices for the weight function are $X^U(u)$, $I_k^D(u)$ and $R \cdot I_l^1(u)$. Let $p_1 = \Pr(D = 1 \mid R = 1)$ and $p_0 = \Pr(D = 1 \mid R = 0)$. The density of U_0 given D and R is denoted by $g_0(u|D, R)$, the hazard and the derivative are defined analogously. Finally, we denote

$$\chi(u \mid D, R) = g_0(u \mid D, R)/\overline{G}_0(u) + g_0'(u \mid D, R)/g_0(u)$$

and

$$\begin{aligned} M_j^D(u|D,R) = &\Big[\frac{g_0(u \mid D, R)}{g_0(u)} + u \cdot \chi(u \mid D, R)\Big] I_j^D(u) - m_{j-1}(\overline{X}, D) \chi(u \mid D, R) I_j^D(u) \\ &+ \chi(u \mid D, R)\Big(m_j(\overline{X}, D) - m_{j-1}(\overline{X}, D)\Big) I\big(u > m_j(\overline{X}, D)\big) \end{aligned} \tag{39}$$

for $j = k = 1, \ldots, K-1$ and for $j = l = 1, \ldots, L$.

[9] Because in a model with $\tilde{\alpha}_k$ defined for each interval only $K-1$ of these α's are identified (see Bijwaard and Ridder (2000)) we choose to fix $\tilde{\alpha}_K = 0$ and redefine the remaining $\tilde{\alpha}$'s in $\alpha_k = \tilde{\alpha}_k - \tilde{\alpha}_K$.

From (22) and (35) we derive the optimal W–function for this model as

$$
\begin{aligned}
W_{\beta,\mathrm{opt}}\big(u,\overline{X}^U(u),R\big) &= X(u)+\frac{\mu_0'(u)}{\mu_0(u)}\int_0^u X(s)\,ds && (40)\\
W_{\alpha_k,\mathrm{opt}}\big(u,\overline{X}^U(u),R\big) &= R\Big[p_1 M_k^1(u\mid 1,1)+(1-p_1)M_k^0(u\mid 0,1)\Big] && (41)\\
&\quad +(1-R)\Big[p_0 M_k^1(u\mid 1,0)+(1-p_0)M_k^0(u\mid 0,0)\Big]\\
W_{\gamma_l,\mathrm{opt}}\big(u,\overline{X}^U(u),R\big) &= R\cdot p_1 M_l^1(u\mid 1,1)+(1-R)p_0 M_l^1(u\mid 1,0) && (42)
\end{aligned}
$$

Note that $W_{\beta,\mathrm{opt}}\big(u,\overline{X}^U(u),R\big)$ is identical to $W_{\beta,\mathrm{opt}}\big(u,\overline{X}^U(u)\big)$ defined in Bijwaard and Ridder (2000). Thus, instrumenting does not effect the optimal weight function of the regression parameters.

If the treatment decision is not confounded we have $g_0(u\mid D,R)=g_0(u)$, $\mu(u\mid D,R)=\mu_0'(u)$. Then, $W_{\alpha_k,\mathrm{opt}}\big(u,\overline{X}^U(u),R\big)$ is almost identical to the optimal W–function for α_k defined in Bijwaard and Ridder (2000). The only difference is that the optimal weight function of the IVLR estimator is the average of $M_k^0(u)=M_k^0(u\mid D=0,R)$ and $M_k^1(u)=M_k^1(u\mid D=1,R)$ while the optimal weight function of the LRE is based solely on $M_k(u\mid D)$.

Finally, we mention that with full compliance in the control group, i.e. $p_0=0$, and a constant treatment effect, the optimal weight function for γ reduces to

$$
W_{\gamma,\mathrm{opt}}\big(u,\overline{X}^U(u),R\big)=R\cdot p_1\Big[g_0(u|1,1)/g_0(u)+u\cdot\chi(u|1,1)\Big] \qquad (43)
$$

(*end of example 1*).

3.3 Estimation in practice

The statistic $S_n(\theta;W)$ is a multi–dimensional step–function. Therefore, the standard Newton–Raphson algorithm cannot be used to solve (26). One of the alternative methods for finding a zero of a non–differentiable function is the Powell–method. This method (see Press et al. (1986, §10.5)) is a multi–dimensional version of the Brent algorithm. Another possible algorithm for finding the minimizer of the quadratic form is Simulated Annealing (see e.g. Vanderbilt and Louie (1984) and Lin and Geyer (1992)).

Related to the computation of optimal weight function is the estimation of the variance matrix for an arbitrary weight function.[10] The difficulty in estimating the covariance matrix lies in the calculation of the matrix $Q(W)$ and not in the calculation of the variance matrix of the estimating equation. The latter can be consistently estimated by

$$
\begin{aligned}
\hat{\Omega}=\frac{1}{n}\sum_{i=1}^{n}\int_0^{C_i^U(\hat\theta)} &\Big[W\big(u,\overline{X}_i^U(u;\hat\theta),\overline{R}_i^U(u;\hat\theta)\big)-\overline{W}(u,\hat\theta)\Big]\\
&\times\Big[W\big(u,\overline{X}_i^U(u;\hat\theta),\overline{R}_i^U(u;\hat\theta)\big)-\overline{W}(u,\hat\theta)\Big]' d\hat{N}_i^U(u) \qquad (44)
\end{aligned}
$$

[10] Robins and Tsiatis (1991) suggested to use a numerical derivative of $n^{-1}S_n(\theta;W)$ that does not need an estimate of the optimal W–function to get $\hat{Q}(W)$. This numerical derivative is sensitive to the choice of the difference in θ. We found it hard to get stable results.

where $\hat{N}_i^U(u)$ is the counting process of $U(\hat{\theta})$.

The computation of the variance matrix and the adaptive IVLR deserve more attention. The difficulty in computing the variance matrix lies in the calculation of the matrix $Q(W)$ and not in the calculation of the variance matrix of the estimating equation. The latter can be consistently estimated by

$$\hat{\Omega} = \frac{1}{n}\sum_{i=1}^{n}\int_0^{\tau}\left[W\Big(u,\overline{X}_i^U\left(u;\hat{\theta}_n(W)\right)\Big) - \overline{W}\big(u,\hat{\theta}_n(W)\big)\right] \\ \times\left[W\Big(u,\overline{X}_i^U\left(u;\hat{\theta}_n(W)\right)\Big) - \overline{W}\big(u,\hat{\theta}_n(W)\big)\right]' d\hat{N}_i^U(u) \quad (45)$$

where $\hat{N}_i^U(u)$ is the counting process of $U\big(\hat{\theta}_n(W)\big)$.

These functions are estimated in two steps. The first step consists of obtaining a consistent estimate of θ_0 using an instrumental function that does not depend on the distribution of U_0. The transformed durations for these parameter values are estimates of the unobserved population transformed durations. The second step concerns the estimation of the unknown distribution of U_0. Many different methods are available to get a reasonable estimate of an unknown distribution. We shall *not* apply the commonly used kernel based method. Although kernel–smoothed hazard rate estimators have been developed (see e.g. Ramlau-Hansen (1983)) and adjusted to deal with the boundary problems inherent to hazard rates (see e.g. Gasser and Müller (1979)) these methods can be difficult to implement due to the choice of the bandwidth. It is also unclear how the boundary corrections can be incorporated in the kernel estimates of the derivative of the hazard. We therefore choose to use a series approximation of the distribution.

Suppose the distribution of U_0 can be approximated arbitrary well using orthonormal polynomials. We base our approximation on Hermite polynomials using the exponential distribution as a weighting function:

$$g_0(u) = \frac{ae^{-au}}{\sum_{j=0}^{J} b_j^2}\left[\sum_{j=0}^{J} b_j L_j(u)\right]^2 \quad (46)$$

where

$$L_j(u) = \sum_{k=0}^{j}\binom{j}{k}\frac{(-au)^k}{k!} \quad (47)$$

are the Laguerre polynomials. The unknown parameters of this approximation are a and $b_0,\ldots,b_J$. If $b_j \equiv 0$ for all $j > 0$ the distribution of U_0 is exponential. Even for J as small as three (46) allows for many different shapes of $\mu_0(u)$ and its derivative. Both can be derived analytically given the estimates of the parameters. The parameter estimators can be obtained from standard maximum likelihood procedures on the observed $(\tilde{U}_i(\hat{\theta}_n(W)), \Delta_i)$.

If a consistent but inefficient estimator $\hat{\theta}_n(W)$ of θ_0 is available and we have estimated the parameters of the polynomial approximation of the distribution of U_0 we can obtain an efficient estimator $\hat{\theta}_{\text{opt}}$ in just one extra step. From the linearization of the estimating equations, given in (30), we obtain an efficient estimator from

$$\hat{\theta}_{\text{opt}} = \hat{\theta}_n(W) - \hat{Q}(W)^{-1} S_n(\hat{\theta}_n(W); W_{\text{opt}})/n \quad (48)$$

It also possible to obtain the efficient estimator directly from minimizing the quadratic form $S_n(\theta; W_{\text{opt}})' S_n(\theta; W_{\text{opt}})$. However, this involves again the minimization of a multi–dimensional step function.

4 Application to the Illinois Reemployment Bonus Experiment

Between mid–1984 and mid–1985, the Illinois Department of Employment Security conducted a controlled social experiment.[11] This experiment provides the opportunity to explore, within a controlled experimental setting, whether bonuses paid to Unemployment Insurance (UI) beneficiaries or their employers reduce the time spend in unemployment relative to a randomly selected control group. In the experiment, newly unemployed claimants were randomly divided into three groups: a *Claimant Bonus Group*, a *Employer Bonus Group* and, a *control group*. The members of both treatment group were instructed that they (Claimant group) or their employer (Employer group) would qualify for a cash bonus of \$500 if they found a job (of at least 30 hours) within 11 weeks and, if they held that job for at least four months. Each newly unemployed individual who was randomly assigned to one of the two treatment groups had the possibility to refuse participation in the experiment.

Woodbury and Spiegelman (1987) concluded from a direct comparison of the control group and the two treatment groups that the claimant bonus group had a significantly smaller average unemployment duration. The average unemployment duration was also smaller for the employer bonus group, but the difference was not significantly different from zero. These results are confirmed in table 4.1. Note that the response variable is insured weeks of unemployment. Because UI benefits end after 26 weeks, all unemployment durations are censored at 26 weeks. In table 4.1 no allowance is made for censoring. In the table we distinguish between compliers and non-compliers. We see that the claimant bonus only affects the compliers and that the average unemployment duration of the non-compliers and the control group are almost equal.

Table 4.1: Average unemployment durations:
control group and (non-)compliers.

	Control Group	Claimant Bonus			Employer Bonus		
		All	Compl.	Non-compl.	All	Compl.	Non-compl.
Benefit weeks	18.33	16.96	16.74	18.18	17.65	17.62	17.72
	(0.20)	(0.20)	(0.22)	(0.50)	(0.21)	(0.26)	(0.35)
N	3952	4186	3527	659	3963	2586	1377

standard error of average in brackets.

About 15% of Claimant group and 35% of the employer group declined participation. The reason for this refusal is unknown. In Bijwaard and Ridder (2002) we showed that the participation rate is significantly related to some observed

[11] More detailed information can be found in Bijwaard (2001). A complete description of the experiment and a summary of its results can be found in Woodbury and Spiegelman (1987).

characteristics of the individuals that also influence that re–employment hazard (see table B.1). Hence, we cannot exclude the possibility of unmeasured variables that affect both the compliance decision and the re–employment hazard.

In Bijwaard and Ridder (2002) we analyzed this data using the Two Stage Linear Rank (2SLR) estimator. The 2SLR is a device to reduce the computational burden by dividing the computation into two steps. This is appealing because in the 2SLR is the solution to a discontinuous estimating function. However, the first stage of the 2SLR estimator is based on a MPH model for the control group and can only be estimated if the individuals in that group do not have any possibility to get a treatment. Although, this is the situation for the data under consideration and we found that the 2SLR removes the biases of the Maximum Likelihood (based on exogeneity of the treatment) and the intention–to-treat (ITT, ML based on the treatment assignment instead of the actual treatment), it restricts the model unnecessary.

In this paper we extend the analysis of Bijwaard and Ridder (2000) to the (possibly) endogenous participation decision. Apart from the effect of the bonus the model is the same as in Bijwaard and Ridder (2000) with a log–linear regression function and a piecewise constant λ. The program effect, the regression parameters and, the parameters of λ are all estimated simultaneously. The choices of the specification of the GAFT model are based on the results of Bijwaard and Ridder (2000). We consider the two interventions separately: thus Claimant Bonus group versus Control group and Employer Bonus group versus Control.

We shall consider two alternative treatment functions: (i) constant treatment effects and, (ii) a change in the effect after 10 weeks, in line with the end of the eligibility period of the bonuses.[12] Thus, the implied transformed durations are

$$U(\theta) = \int_0^T \lambda(s;\alpha) e^{\beta' X + (\gamma_1 I_1(s) + \gamma_2 I_2(s))D} ds \tag{49}$$

with $I_1(t) = I(0 \leq t < 11)$ and $I_2(t)$ is its complement. Note that the covariates are all time–constant because the individual characteristics available in the data are all determined when the individuals register at the unemployment office. We include the following: the logarithm of the age (LNAGE), the logarithm of the pre–unemployment earnings (LNBPE), gender (MALE= 1), ethnicity (BLACK= 1), and the logarithm of the weekly amount of UI benefits plus dependence allowance (LNBEN). We employ three different specifications for $\lambda(t;\alpha_0)$: (i) AFT model, *i.e.* $\lambda(t;\alpha_0) \equiv 1$; (ii) model with a piecewise constant λ on five intervals 0–2, 2–4, 4–10, 10–25 and 25 and beyond; (iii) model (ii) in which the third interval is split in two 4–6 and 6–10.

For identification we need to set one of the parameters of the piecewise constant λ equal to one (or the log equal to zero). We call the interval on which the value of λ is one the base interval. In the two specifications of λ with more than one interval we let the base interval start on the last week before the end of the observation period, at 25 weeks. This is done for two reasons. First, it is easier to compare the estimates of the λ for different numbers of intervals. Second, the spike in the observed unemployment duration just before the UI

[12]For both the interventions the treatment path is determined at the start. It should be underlined that the estimated treatment effects correct for bias due to selective non–participation. No attempt is made to estimate the effects under the (counterfactual) assumption that all eligible individuals indeed collect the bonus.

eligibility period ends is captured. This is also the end of the observation period and, therefore, for all individuals the potential censoring time is at 26 weeks. We denote by α_k the log values of λ in the relevant interval minus the log value in the base interval.

For all specifications we estimate a first stage IVLR using the Powell–method and the one step optimal IVLR. The first stage IVLR uses the values of the covariates, the interval indicators on the transformed duration and, the treatment assignment indicator times the treatment interval indicators on the transformed duration, $R \cdot I_1(u)$ and $R \cdot I_2(u)$, as the weight functions for the parameters. From these first stage IVLR's the implied transformed duration are obtained. Then, we estimate the parameters of the polynomial approximation of the distribution of U conditional on R and D as mentioned in section 3.2 (and described in detail in section 3.3). From these estimated parameters we can calculate the hazard and its derivative of the transformed duration.

Example 1 can help to deduce all the relevant optimal weight functions. In particular, we can substitute the estimated distributions (and implied derivatives and hazards) of U_0 given D and R into (39) and subsequently substitute all of these in (40), (41) and (42), to get all the optimal weight functions. Note that in this application $p_0 = 0$ and, therefore, $M_k^1(u \mid 1,0) = M_k^0(u \mid 0,0)$. This full compliance in the control group also allows us to apply (43) for the model with constant treatment effects.

The results for the treatment effects are reported in table 4.2. The parameter estimates of the regression parameters are reported in table B.2 (for the model with constant treatment effects) and in table B.3 (for the model with time–varying treatment effects). Finally, table B.4 gives the estimated parameters of λ. These last three tables can be found in appendix B.

The Coase Theorem predicts that in a world of zero transaction costs the bonuses paid to the Claimant group and to the Employer group would be equally efficient. Because they both imply the same amount of money, an employment contract would be established whenever the bonus was sufficient to enable a mutually advantageous bargain to be struck. However, Woodbury and Spiegelman (1987) concluded from a direct comparison of the control group and the two treatment groups that the claimant bonus group had a significantly smaller average unemployment duration.[13] The average unemployment duration was also smaller for the employer bonus group, but the difference was not significantly different from zero.

We cannot confirm the different treatment effects of the two bonuses if we use the IVLR estimator of the GAFT model. Although we find for almost all specifications a slightly greater effect on the re–employment probability of the *Claimant Bonus*, these effects do not differ significantly from the effects of the *Employer Bonus*. For both groups the results in table 4.2 indicate that the estimated treatment effects are decreasing if we allow for a more flexible λ. The same pattern is observed for the (absolute) values of the regression parameters in table B.2 and B.3. The AFT model seem to overestimate the treatment effects. This is especially apparent if we allow the treatment effect to change after ten weeks of unemployment duration.

The results clearly indicate that the bonuses only influence the chances to find a job in the first ten weeks. This is in line with the bonus eligibility period:

[13] See also Bijwaard and Ridder (2002), table 4.1 and, Donohue (1989).

Table 4.2: Instrumental Variable Linear Rank estimates for the effect of the Bonus

Constant effect		*Claimant*			*Employer*	
	AFT	(2)	(3)	AFT	(2)	(3)
First stage	0.1446	0.1141	0.1024	0.1011	0.0866	0.0721
	(0.0493)	(0.0369)	(0.0523)	(0.0646)	(0.0496)	(0.0470)
1–step optimal	0.1596	0.1004	0.0932	0.1332	0.1100	0.0696
	(0.0460)	(0.0355)	(0.0380)	(0.0612)	(0.0459)	(0.0425)
Time varying effect						
		Claimant			*Employer*	
First stage	AFT	(2)	(3)	AFT	(2)	(3)
0-10	0.2955	0.1593	0.1433	0.2304	0.1333	0.1103
	(0.0523)	(0.0624)	(0.0907)	(0.0710)	(0.0553)	(0.0736)
10+	-0.0720	0.0166	0.0063	-0.0783	0.0207	-0.0048
	(0.0608)	(0.0682)	(0.0886)	(0.0836)	(0.1116)	(0.1253)
1–step optimal						
0-10	0.3865	0.1468	0.1439	0.6334	0.1450	0.1279
	(0.0486)	(0.0502)	(0.0578)	(0.0674)	(0.0568)	(0.0521)
10+	-0.0437	-0.1195	-0.0411	0.0330	0.0060	-0.0747
	(0.0572)	(0.0652)	(0.0850)	(0.0745)	(0.0929)	(0.0882)

(2) piecewise constant intervals: 0–2, 2–4, 4–10, 10-25, 25 →;
(3) piecewise constant intervals: 0–2, 2–4, 4–6, 6–10, 10–25, 25 →;
standard error in brackets.

those who find a job after that period would not get the bonus. However, the increase in the treatment effect if it is confined to this bonus eligibility period is not as large as we found before (in Bijwaard and Ridder (2002)). The effect of the Claimant Bonus increases from about 10% higher probability to find a job at every unemployment duration to about 15% higher probability to find a job in the first ten weeks (and no effect thereafter). While the bonus for the Employer group raises the job finding probability with about 7% at every unemployment duration or with about 12% in the first ten weeks of unemployment.

The AFT models tells a different story, but we tend to reject this model. Another indication that λ should be included is the difference between the first stage and one–step optimal estimators for the AFT model. For a correctly specified model both estimators are consistent and, therefore, do not differ much. In the models with a λ specified the first stage and one–step estimator are of the same magnitude. The estimated standard errors of the latter are, as expected, substantially lower in most situations.

Although the focus in this article is on the estimation of the effect of a possibly confounded intervention we also give a short discussion on the other estimated parameters. These estimators can be found in the tables in appendix B. We mentioned already that the regression parameters are overestimated (in absolute terms) if we assume an AFT model. These regression parameters hardly change from a model with constant treatment effects (table B.2) to a model with time–varying treatment effects (table B.3). The regression parameters for the Claimant data and the Employer data (both including the control group) are almost identical. Gender, MALE, is the exception; This covariate has no significant influence on the re–employment probability in the Employer data. The shape of the estimated λ's are very similar to those we obtained in Bijwaard and Ridder (2000) on the control group. Again the results indicate a U–shaped λ.

We end with a discussion on the selectivity in the bonus data. The compliance rate in the Claimant group, 85%, was much higher than the compliance rate in the employer group, 65%. Many individuals in the Employer group, apparently and contrary to our findings, did not perceive a bonus paid to their new employer beneficiary for their job search. Following Moffitt (1983) this partial compliance may be explained by a stigma effect. However, this is a tentative explanation because our analysis only adjust for (possible) selective compliance. It does not provide a model for the selection process. Thus, both an advantage and a drawback of our method is that we do not make any assumptions on the selection process and therefore cannot tell why individuals make such a selective decision.

5 Conclusion

In this article we extend the analysis of the GAFT model in Bijwaard and Ridder (2000) to allow for endogenous covariates. In particular, we focus on time-to-event studies with a randomised treatment assignment that may be compromised by selective compliance. We show that even if the compliance is selective, we can still use the randomisation in the assignment to the intervention and control groups to estimate the impact of the program on the hazard correcting for selective compliance. The only requirement is that participation in the program is affected by a variable that is not correlated with the baseline duration.

We develop an Instrumental Variable estimation procedure for this model that is closely related to the Rank Preserving Structural Failure Time Model (RPSFTM) estimator of Robins and Tsiatis (1991). The difference is originated by the basic model they assume if there is no instrumenting: the models of Robins and Tsiatis (1991) collapse into the strong version of the AFT model, while our models collapse into the GAFT models proposed in Bijwaard and Ridder (2000). The empirical application shows that incorrectly assuming an AFT model can give misleading conclusions about the effects of a bonus on the re–employment hazard. We discuss the large sample properties of this Instrumental Linear Rank Estimation and show how we can improve its efficiency.

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Appendices

A Asymptotic properties of the IVLR

In this appendix we prove the consistency (theorem 1) and asymptotic normality (theorem 2) of the IVLR. The proof of the consistency and asymptotic normality are both based upon the asymptotic linearity of $S_n(\theta; W)$ in the neighbourhood of the true value θ_0. We follow the reasoning of Tsiatis (1990). The following lemma shows that the linearization in (30) is uniformly close to the original estimating function

Lemma 1. *In neighbourhoods of $O(n^{-1/2})$ of θ_0*

$$n^{-1/2} \left\| \tilde{S}_n(\theta; W) - S_n(\theta; W) \right\|$$

converges uniformly to zero.

This lemma implies that $n^{-1/2}\tilde{S}_n(\theta; W)$ and $n^{-1/2}S_n(\theta; W)$ are asymptotically equivalent in a neighbourhood close to θ_0.

Proof: This can be proved in lines of Tsiatis (1990) Lemma (3.1) and (3.2) and theorem (3.2) and this is, because of the analogy, not repeated here.

Proof of theorem 1 and theorem 2. According to lemma 1 is $n^{-1/2}S_n(\theta; W)$ in a neighbourhood close to θ_0 asymptotically equivalent to $n^{-1/2}\tilde{S}_n(\theta; W)$. Then the estimates θ^* and $\hat{\theta}$, with $\tilde{S}_n(\theta^*; W) = 0$, will also be asymptotically equivalent. Clearly, θ^* converges in probability to θ_0. Hence, if we show that $\sqrt{n}(\hat{\theta} - \theta^*) \xrightarrow{p} 0$ then this would imply that $\hat{\theta}$ also converges in probability to θ_0. Tsiatis (1990) argues that lemma 1 suffices to proof this. This proves theorem 1.

According to the Mann–Wald theorem convergence in probability implies convergence in distribution. We note that $\sqrt{n}(\theta^* - \theta_0) = n^{-1/2}Q^{-1}(W)S_n(\theta_0; W)$ clearly converges to a normal distribution with mean zero and variance matrix $Q^{-1}(W)\Omega(W){Q'}^{-1}(W)$. This completes the proof of theorem 2. $\square$

Remark. To establish detailed conditions on when $\tilde{S}_n(\theta; W)$ has a unique root is rather tedious; however Ying (1993) gave an excellent general treatment on rank estimation, which can also be used for the estimating equations in this chapter.

B Additional Tables for the IVLR of reemployment bonus experiment

Table B.1: Probit analysis of the compliance decision.

	Claimant Bonus	Employer Bonus
Constant	0.9973	0.3108
	(0.0384)	(0.0329)
AGE	-0.0029	-0.0025
	(0.0027)	(0.0024)
LNBPE	0.0763	-0.1328
	(0.0544)	(0.0559)
BLACK	-0.1866	0.0565
	(0.0527)	(0.0484)
MALE	0.1073	0.1360
	(0.0478)	(0.0421)
LNBEN	-0.1973	-0.1535
	(0.0958)	(0.0939)
Log likelihood	-1810.21	-2524.71
LR test (5 d.f.)	24.60	69.69
N	4186	3963
No. of compliers	3527	2586

Table B.2: Instrumental Variable Linear Rank estimates for the regression coefficients of the Illinois data (Constant Bonus Effect)

First stage		*Claimant*			*Employer*	
	AFT	(2)	(3)	AFT	(2)	(3)
LNAGE	-0.5718	-0.3807	-0.3424	-0.5219	-0.4053	-0.3379
	(0.0734)	(0.0780)	(0.0897)	(0.0717)	(0.0668)	(0.0699)
LNBPE	0.3528	0.2388	0.2146	0.3188	0.2446	0.2036
	(0.0510)	(0.0534)	(0.0601)	(0.0512)	(0.0468)	(0.0482)
BLACK	-0.6636	-0.4211	-0.3770	-0.6264	-0.4587	-0.3792
	(0.0526)	(0.0684)	(0.0842)	(0.0510)	(0.0557)	(0.0641)
MALE	0.1135	0.0740	0.0663	0.0464	0.0358	0.0295
	(0.0377)	(0.0320)	(0.0330)	(0.0376)	(0.0309)	(0.0305)
LNBEN	-0.5841	-0.3956	-0.3558	-0.6263	-0.4823	-0.4010
	(0.0867)	(0.0897)	(0.1011)	(0.0871)	(0.0826)	(0.0865)
One step Optimal						
		Claimant			*Employer*	
	AFT	(2)	(3)	AFT	(2)	(3)
LNAGE	-0.5204	-0.3711	-0.3612	-0.4733	-0.3509	-0.3110
	(0.0693)	(0.0656)	(0.0653)	(0.0683)	(0.0613)	(0.0603)
LNBPE	0.3537	0.2325	0.2266	0.3133	0.2108	0.1871
	(0.0473)	(0.0452)	(0.0449)	(0.0483)	(0.0434)	(0.0424)
BLACK	-0.6162	-0.4085	-0.3982	-0.5646	-0.3992	-0.3574
	(0.0509)	(0.0507)	(0.0510)	(0.0495)	(0.0448)	(0.0443)
MALE	0.1293	0.0713	0.0691	0.0698	0.0272	0.0227
	(0.0355)	(0.0306)	(0.0303)	(0.0355)	(0.0309)	(0.0303)
LNBEN	-0.5924	-0.3807	-0.3692	-0.6040	-0.4114	-0.3610
	(0.0813)	(0.0766)	(0.0762)	(0.0826)	(0.0745)	(0.0727)

(2) piecewise constant intervals: 0–2, 2–4, 4–10, 10-25, 25 →;
(3) piecewise constant intervals: 0–2, 2–4, 4–6, 6–10, 10–25, 25 →;
standard error in brackets.

Table B.3: Instrumental Variable Linear Rank estimates for the regression coefficients of the Illinois data (Time–varying Bonus effect)

First stage		*Claimant*			*Employer*	
	AFT	(2)	(3)	AFT	(2)	(3)
LNAGE	-0.5361	-0.3692	-0.3285	-0.5233	-0.3906	-0.3355
	(0.0693)	(0.0826)	(0.0897)	(0.0706)	(0.0702)	(0.0763)
LNBPE	0.3313	0.2388	0.2139	0.3153	0.2447	0.2029
	(0.0481)	(0.0570)	(0.0617)	(0.0506)	(0.0498)	(0.0530)
BLACK	-0.6086	-0.4080	-0.3665	-0.6268	-0.4467	-0.3771
	(0.0494)	(0.0749)	(0.0861)	(0.0501)	(0.0612)	(0.0740)
MALE	0.1036	0.0740	0.0668	0.0461	0.0362	0.0294
	(0.0352)	(0.0328)	(0.0337)	(0.0371)	(0.0308)	(0.0304)
LNBEN	-0.5470	-0.3956	-0.3564	-0.6187	-0.4819	-0.3989
	(0.0820)	(0.0958)	(0.1043)	(0.0859)	(0.0885)	(0.0959)
One step Optimal						
		Claimant			*Employer*	
	AFT	(2)	(3)	AFT	(2)	(3)
LNAGE	-0.4861	-0.3578	-0.3288	-0.4529	-0.4057	-0.3660
	(0.0653)	(0.0642)	(0.0664)	(0.0675)	(0.0623)	(0.0622)
BPE	0.3332	0.2247	0.2061	0.3017	0.2433	0.2236
	(0.0442)	(0.0442)	(0.0455)	(0.0474)	(0.0437)	(0.0434)
BLACK	-0.5644	-0.3987	-0.3615	-0.5286	-0.4594	-0.4189
	(0.0476)	(0.0502)	(0.0533)	(0.0488)	(0.0467)	(0.0480)
MALE	0.1176	0.0689	0.0626	0.0622	0.0334	0.0283
	(0.0332)	(0.0303)	(0.0304)	(0.0349)	(0.0307)	(0.0302)
LNBEN	-0.5501	-0.3675	-0.3343	-0.5813	-0.4737	-0.4284
	(0.0765)	(0.0751)	(0.0770)	(0.0815)	(0.0755)	(0.0752)

(2) piecewise constant intervals: 0–2, 2–4, 4–10, 10-25, 25 →;
(3) piecewise constant intervals: 0–2, 2–4, 4–6, 6–10, 10–25, 25 →;
standard error in brackets.

Table B.4: Estimated λ for the Bonus data

Claimant	interval	*Constant Bonus effect* first	opt.	*Time varying Bonus effect* first	opt.
(2)	0–2	0.5653	0.6758	0.6143	0.7822
		(0.3616)	(0.2197)	(0.4316)	(0.2156)
	2–4	0.1180	0.1726	0.1381	0.1542
		(0.2944)	(0.1636)	(0.3391)	(0.1610)
	4–10	-0.2988	-0.1639	-0.2855	-0.1476
		(0.1996)	(0.1255)	(0.2393)	(0.1254)
	10–25	-0.7121	-0.6717	-0.6585	-0.5487
		(0.1511)	(0.1026)	(0.1563)	(0.0907)
(3)	0–2	0.8098	0.7500	0.8625	0.9328
		(0.4638)	(0.2052)	(0.5262)	(0.2409)
	2–4	0.3146	0.2348	0.3542	0.2309
		(0.3691)	(0.1462)	(0.4048)	(0.1799)
	4–6	-0.0782	-0.0415	-0.0390	0.0318
		(0.2646)	(0.1220)	(0.3015)	(0.1552)
	6–10	-0.2743	-0.1859	-0.2341	-0.2085
		(0.2392)	(0.1133)	(0.2807)	(0.1369)
	10–25	-0.6868	-0.6655	-0.6077	-0.6345
		(0.1626)	(0.1006)	(0.1758)	(0.1261)
Employer	interval	*Constant Bonus effect* first	opt.	*Time varying Bonus effect* first	opt.
(2)	0–2	0.2781	0.6903	0.3029	0.3593
		(0.2565)	(0.1407)	(0.3358)	(0.1681)
	2–4	-0.0925	0.2458	-0.0811	-0.0341
		(0.1865)	(0.0965)	(0.2583)	(0.1202)
	4–10	-0.4944	-0.3020	-0.4927	-0.3249
		(0.1474)	(0.0676)	(0.1860)	(0.0893)
	10–25	-0.7908	-0.6793	-0.7688	-0.6358
		(0.1013)	(0.0374)	(0.1035)	(0.0470)
(3)	0–2	0.7095	0.8929	0.7088	0.5647
		(0.3063)	(0.1450)	(0.4375)	(0.1716)
	2–4	0.2540	0.4451	0.2542	0.1464
		(0.2134)	(0.0939)	(0.3344)	(0.1227)
	4–6	-0.1217	-0.1178	-0.1195	0.0875
		(0.2008)	(0.0925)	(0.2330)	(0.1050)
	6–10	-0.4552	-0.2707	-0.4526	-0.4098
		(0.1516)	(0.0751)	(0.2255)	(0.0975)
	10–25	-0.7492	-0.6826	-0.7180	-0.6057
		(0.0971)	(0.0372)	(0.1015)	(0.0491)

(2) baseline hazard intervals: 0–2, 2–4, 4–10, 10-25, 25 →;
(3) baseline hazard intervals: 0–2, 2–4, 4–6, 6–10, 10–25, 25 →;
standard error in brackets.